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Sent: Tuesday, June 17, 2008 3:07 PM
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Subject: Food Web Model Comments
Attachments: Portland Harbor FWM; BruceHComments R2 App H.doc; PH FW model Bruce H comments.htm; AppendixEComments061208_JPComments.doc

Eric and Burt,

I have added my comments to Eric's, which was a good start to the comments. I know we have been busy with other things, but this model really needs to be run correctly to meet project objectives. I am concerned, because it doesn't appear that the meetings of a year or so ago resulted in an agreed upon product. The model is over calibrated, and the uncertainty and sensitivity analysis so limited they seem useless. Despite schedule demands, I hope we can given this the attention it needs to produce a good tool for decision making at the site. I hope we can discuss before the comments go to LWG. I am also attaching Bruce's comments - I am sure you got these but they didn't make it into the comments that were pulled together so I thought I would include just in case. I would also like to fax you a summary from Gobas when you get back in the office. Again, here is a re-cap of my biggest issues:

1. Uncalibrate the model. Run the model forward to evaluate observed versus predicted values in fish tissue for all samples across the harbor (not as an average).

2. Focus on congeners, and revisit the list used for modeling. The congeners they have selected do not represent the ones that represent the most risk or in some cases doesn't even represent ones that were detected in fish tissue with the most frequency. We need to understand the implications of modeling mixtures, but more importantly we have to know that the model works for individual chemicals, because that is the only "real" data showing sediment, water and tissue distributions. This should focus on at least some TEQ congeners.

3. Refine how water is used in the model. We need to get this right because it is a very sensitive parameter for the model, and sediment / water contributions will likely be a topic of debate in the project. Organic carbon is an important partitioning phase, but the way the water data available at the site should be used in the food web model has not been resolved. Our dissolved filter was 0.5 um - quite a bit larger what would be used for truly dissolved or bioavailable (0.2 um). Dissolved organic carbon is considered to be comprised of particles smaller than 0.45 um diameter. We made a comment a few iterations ago that the empirical data should be considered in model development / calibration, and they responded by removing total water values from the model entirely, and no overlying water was used. This issue needs further discussion, but the result is that the water data used for the model was limited to only water stations that collected filtered XAD values. Only 3 water transects were used in the model (integrated). This is also not consistent with the most recent model by Gobas (2004). The result of this change, as stated by the LWG, is that the bioavailable concentration in water is reduced by 1/3 (see e-mail from Nancy to Bruce H). Also, other bioavailable terms in the model are no longer used (e.g. POC). I am not sure why the use of empirical data would modification using other equation (one that doesn't even match the original citation of Morrison 1997 (see page 5 of attachment E1), but we may need to consider returning to the original equations.

4. Move away from using standard errors on mean data as distributions. The focus on the mean misses the whole point of including uncertainty / sensitivity analysis does not give us the information we need on variability and uncertainty in the empirical data and resulting model predictions. The approach used here is really no better than selecting point estimates for each parameter.

Additional Comments:

Temperature: They used a mean temperature of 13.6 C, and only varied this parameter relative to the standard error on the mean. The full distribution of temperature should be used (see comment above). This is a sensitive parameter, so we need to have a path forward. If we want to move away from distributions to describe the sensitivity of this parameter we should pick an upper confidence, as we did for the fish dietary approach.

TSS: All data should be used, not just near bottom.

Dissolved organic carbon and Water chemistry: Only the standard error on the mean was used in the sensitivity analysis. Distributions of all the data should be used, or we should settle on an appropriate deterministic value that is an upper confidence.

Sediment Data: Move away from input parameters of SWAC values for organic carbon and concentration. Present as a distribution. Do not use Thiessen polygons to estimate sediment TOC and chemicals concentrations in the surface sediment. Use distributions of the empirical data. Include distributions of sediment in calibration. The LWG states "because the primary purpose of model development for this report was generation of iPRGs, the uncertainty surrounding estimates of sediment concentration was not a primary concern of model calibration." We have to have confidence in model predictions in order to use it for management decisions. Included in this must be an estimate of uncertainty surrounding the sediment concentration used in the model.

Porewater Ventilation: This variable is important for those that truly ingest porewater because the porewater is at a higher fugacity than the overlying water. However, the LWG states "the fraction of porewater ventilated by each species was determined by best professional judgment". Table 6-2 (Attachment E3) shows that the best professional judgment was not conservative or relevant. Refer to Gobas values for the benthic invertebrate detrital/deposit feeders for a more realistic value for true infaunal species - it is for these species that this value is truly important. 0.05 was selected by the LWG from a range of 0.01 to 0.1. Are they even coming close to predicting empirical Lumbriculus tissue using this approach?

Dietary Assumptions: These need to be looked at closely and a proper sensitivity analysis should be conducted. There are some weird diets in here and notably some "professional judgment". The "benthic invertebrate consumer" is likely models conc. in something like Lumbriculus in a total underestimate of reality, and yet for sculpin "BIC" makes up 90% of the diet (LWG says "fish consumption transferred to clams, worms and crayfish"). Why not just use sculpin tissue concentrations (e.g. they eat each other and other small fish like them)? Benthic fish (e.g. carp and largescale sucker) are only eating 10% clams? Carp are 45% phytoplankton and 45 BIC? Northern pikeminnow are eating mostly crayfish (40%) and some sculpin (25%) but not other higher trophic level fish?

Table 1 -3, Attachment E2: These tables show tissue concentration for PCB congeners 17, 170 and 206 for tissue, sediment and water. However, only the means are shown. We need understand and use the range of values in the harbor in the model. No SWACs!

Tables 4-1 through 6-2 (Attachment E3): Move away from using the standard error on the mean to represent a distribution! If we are going to do this, pick one reasonable value because the analysis is useless.

Table 2 (Attachment E4): The table here shows that individual PCB congener (e.g. PCB 17, 170 and 206) concentrations used in the model for fish tissue were calculated from a sum of Aroclors for sculpin. How? Also, a Total Dioxin Like TEQ value (that would include the total TEQ from a mixture of 2,3,7,8-TCDD equivalence concentrations of an individual PCDD,PCDF and PCB congeners. Since the PCB TEQ concentrations are higher than the Dioxin TEQ numbers in Table 2, it is clear that the combination is not represented at present. Also, fish should be added to the list for TEQ summations in addition to birds and mammals.

-Jennifer